“A dynamic and thermodynamic analysis of the 11 December 2017 tornadic supercell in the Highveld of South Africa”

Response to reviewer #2

Reviewer: Lekoloane et al. provide an interesting case study and a pair of model simulations associated with the 11 December 2017 tornadic supercell in the Highveld of South Africa. While this case would be of interest to the community, I have some serious reservations about the manuscript in its current form. In particular, there appear to be some fundamental misunderstandings—or at least omissions—related to the current knowledge base of supercell tornadogenesis. Additionally, I am concerned that the analyses of high-resolution simulations are too limited in scope in their current state and also reveal some misunderstandings. These issues could be alleviated with considerable revision and reanalysis, but I do not think the manuscript is suitable for publication at this time.

Authors: Thank you so much for taking the time to review and give feedback to our manuscript, which has led to an improved one. In the revised manuscript, we address all the major and minor comments the reviewer made, including those raised regarding tornadogenesis and model analysis. Below we make a point-by-point response to the reviewer’s comments.

Major Comments

Reviewer: 1. Lines 57-59: Your discussion about near-surface vertical vorticity is short and potentially contains some misunderstandings. Tilting of environmental horizontal vorticity is the primary contributor to updraft rotation within a supercell, but it is not typically thought to contribute significantly to near-surface vertical vorticity. The other process that you mention could achieve near-surface vertical vorticity (“advection of vorticity from aloft”) is vague, and I cannot be certain that what you mean here is actually consistent with current thinking. Please revise this section for clarity, and if necessary, review recent literature on the topic to ensure your introduction is consistent.

Authors: Thank you for the observation you make here, and we can see how our two sentences can cause confusion. Rather than go on mentioning more literature on tornadogenesis, which is a really complex and still less understood process (therefore potentially contradicting literature), we thought it best to remove the two sentences you mentioned here and other possible allusions to them, primarily because in this study our main focus is not on tornadogenesis itself, rather the supercell of which in our case was associated with a tornado. Hence, we mostly focused on the tornado in our observation analysis and only supercell in our model analysis in order to see if the configurations used in an operational setting at the South African Weather Service

(SAWS) were able to capture the supercell. This is because we acknowledge that the 1.5 km and 4.4 km models analysed in this study are too course to capture a tornado or even to do an analysis of tornadogenesis, but within a good range of capturing a supercell storm as found in a study by Weisman et al. (1997). Also, for your interest, our statements here were based on the referenced review paper by Markowski and Richardson (2009). Here’s a direct quote from the paper: “By definition, tornadogenesis requires that large vertical vorticity arises at the ground. If preexisting vertical vorticity is negligible near the ground, then vorticity stretching near the ground is initially negligible and vertical vorticity first must arise either from the tilting of horizontal vorticity or from advection toward the surface from aloft.” We understand this might be outdated given that more tornadogenesis studies have been done since then, and new theories developed.

Reviewer: 2a. Your method of evaluating model simulations is potentially flawed. SA1.5 very clearly lacks a supercell, as there was no precipitation present in the specific time frame. Thus, the use of midlevel vorticity was a strange choice to determine the presence of a supercell (or lack thereof). Additionally, you look at one hour here; could the higher-resolution simulation simply be delayed in time? What happened beyond 16z? This analysis seems to be somewhat cursory, and perhaps too superficial for a peer-reviewed manuscript.

Authors: In our revised manuscript we address this by including a figure of the 24-hour period accumulated precipitation spatial distribution for both model configurations. It does indicate that SA4.4 captured the storm while SA1.5 seems to be a case of missed convective initiation. We also include the Integrated Multi-satellitE Retrievals for Global Precipitation Measurement (GPM) (IMERG) and reanalysis (ERA5) precipitation fields which agree well with SA4.4 than SA1.5. We understand that reanalysis is not true observations, but is included to supplement satellite GPM observations. SA1.5 does not simulate any kind of precipitation throughout our analysis period. This was mentioned initially in the manuscript, however we did not show it. This seems to be a case of missed convective initiation, a similar problem found in a previous study by Keat et al. (2019) who also used the SA1.5. Keat et al. (2019) found that it might be due to physics set-up and the way sub-grid processes are parameterised which might not be suitable for South Africa. Since SA1.5 does not capture any storm in our domain during our 24-hour analysis period, we wanted to give possible reasons albeit with no proper reviewed-paper references, we fix this.

Reviewer: 2b. Your comment on lines 342-343, “…as a result of the mid-level vorticity being greatly underestimated, which led to the storm not being initialized” shows a fundamental misunderstanding of the source of modeled midlevel vorticity, or at least inconsistency with your analysis of SA4.4. The midlevel vorticity maxima/minima couplets in these simulations appear to be a consequence of the development of convection, not a cause. Please re-evaluate this statement.

Authors: Thanks for noting this, and we agree that the midlevel vorticity maxima/minima is a consequence of convection and not a cause. We remove this statement especially because SA1.5 failed to initiate any storms. We explain the probable cause by looking at MFC which is a good predictor of storm initiation. We also include reference of a study by Keat et al. (2019) which may explain why SA1.5 was a case of missed convective initiation.

Reviewer: 2c. Likewise, I have some concerns about your analysis and interpretations of SA4.4. As you noted, this simulation likely did not resolve the mesocyclone adequately, which is expected given the fairly coarse resolution (for a CAM). There are also some claims not entirely supported by the data provided, discussed below.

Authors: We agree that a grid length of 4.4km is not sufficient to resolve the mesocyclone and our study adds to other studies that have indicated that higher resolution is needed to resolve some processes in thunderstorms. A study by Weisman et al. (1997) indicated that a 4 km grid spacing model may be sufficient to reproduce mesoconvective circulations and net momentum and heat transport of midlatitude type convective systems. Other studies have however argued that models of grid spacing of 1 km or less are the ones adequate to represent dynamics and local processes responsible for triggering convection (see Roberts, 2008, and Bryan et al., 2003).

Reviewer: "…it follows that the stronger the low-level MFC, the stronger the mid-level vorticity would be." While this is likely just consequence of convergence below an updraft and the corresponding updraft, I'm not sure that the data support this claim in a more general sense. Do you have more evidence?

Authors: Yes we do have a figure which we now include. It indicates a positive correlation between low-level MFC and mid-level vorticity, and that low-level MFC occurs (or intensifies) about 2 to 3 hours prior to the development (or intensification) of mid-level vorticity. We now also note this as a result in our study not necessarily in a more general sense.

Reviewer: Lines 317-318: Given that SA4.4 doesn't adequately resolve the mesocyclone, I don't think you can expect it to accurately represent downdraft processes. RFDs tend to be considered dynamic features associated with low-level rotation within the storms. If the rotation isn't being resolved, I don't think the RFD is being (accurately) represented, either. You may be getting a similar solution for different reasons.

Authors: We agree that the model resolution may be too course to resolve the mesocyclone, and that RFDs are thought to be dynamic features associated with low-level rotation within supercells as also comprehensively reviewed by Markowski (2002) We have included in the manuscript the effect of resolution on our results.

Reviewer: How did you assess the causation of the claim in lines 346-347 ("…found to be a result of…")?

Authors: We compared observed vertical shear and modelled vertical shear. We also looked at simulated MFC, and now include reanalysis MFC to represent observations.

Reviewer: 3. A hook echo does not necessarily confirm that a storm is a supercell, as is suggested in the paragraph beginning on line 236. Instead, focus on the rotation observed from radar velocity fields; the mesocyclone is the key defining feature of the supercell.

Authors: Noted. Thanks for this great suggestion. In our revision we focus on the observed rotation from radar velocity fields.

Reviewer: 4. You seem to provide some details about storm initiation, evolution, and key ingredients in the summary and conclusion section that are not addressed earlier in the manuscript. Please discuss the evolution from a multicell cluster into a supercell and the importance of the "three ingredients" noted beginning at line 330 earlier in the manuscript, if you'd like to keep this section.

Authors: We realised we missed to include the ingredients in a summarised format after our mesoanalysis. We now include them. It was found that the three ingredients that were important in strengthening and maintaining this supercell are: significant surface to midlevel vertical shear, an abundance of low-level warm moisture influx from the tropics and Mozambique Channel, and mid-level lapse rates.

Reviewer: 5. In several locations throughout the manuscript, there were brief, almost random mentions of climate change and the potential increase of extreme events in the future as a result. However, this line of thinking is never appreciably explored or elaborated upon; because of this, I think these mentions should be omitted.

Authors: Thanks for your suggestion, these are removed.

Reviewer: 6. In my opinion, the two paragraphs beginning at line 288 provide no additional insight. These appear to just be confirming the presence of an updraft through show-and-tell. If there is nothing more substantive to include, I would omit these paragraphs.

Authors: Thank you very much for your thought. This is considered.

Less Substantive Comments

Reviewer: 1. Why did you analyze such a small number of the available fields from ERA5? There are 137 vertical levels, and you only use three here. Could any insight be gained by interrogating additional fields/levels?

Authors: Our original thinking was not to include more fields from reanalysis. But as explained in our previous responses, we now think reanalysis could help supplement some observations, and therefore we include more fields when performing the model evaluation.

Reviewer: 2. What was your reasoning for using the convergence of moisture flux at 800 mb rather than the lifted condensation level (LCL) as an assessment of low-level moisture?

Authors: We wanted to examine the lowest pressure level above the surface, which in our case was 800 hPa. For this case, moisture transport into South Africa is influenced by a South Atlantic anticyclone that extends eastward to induce an low level onshore flow into the eastern parts of the country from the South West Indian Ocean. This process is referred to as the ridging high. Therefore, the reason we did not use the LCL was due to the nature of the moisture transport by the ridging high pressure system, which is also influenced by the complex terrain in the eastern parts of the country.

In addition, our approach was to analyse the low-level moisture in general, rather than a specific level, we just didn't include other levels. To give more compelling evidence, in our revised manuscript we now integrate MFC from the surface to 600 hPa, instead of just 800 hPa. This follows Ndarana et al. (2020) who analysed the flow and moisture fluxes associated with ridging South Atlantic Ocean anticyclones during the subtropical southern African summer by integrating moisture fluxes from the surface to 600 hpa "because the flow over the Indian Ocean that is associated with ridging highs changes completely beyond this level to become sinusoidal and westerly, even as the flow in the lower levels is south-easterly." This insures that we analyse the entire low-level MFC. The new MFC equation reflecting the low-level convergence of moisture fluxes builds on equation 5 to become,

$$MFC^* = -\frac{1}{g}\int_{600}^{p_s} \nabla \cdot (q\bar{V}_h)dp$$

Reviewer: 3. For the paragraph beginning at line 206, it would be helpful to refer back to Fig. 1 to help illustrate the storm track.

Authors: Noted. Thanks!

Reviewer: 4. A hodograph corresponding to the Irene sounding would be very helpful.

Authors: Noted. In our revision we include it.

Reviewer: 5. The 35 kt “lower mid-level jet” appears to be a bit stronger than analyzed in Fig. 2. Did this play a role?

Authors: Perhaps, but it would be difficult to confidently say so in our study as it seems to be a common feature and would therefore require a climatological baseline which is currently not available.

Reviewer: 6. Line 230: Why 573 hPa? Is there some specific importance of this level, or is it arbitrary?

Authors: This is arbitrary.

Reviewer: 7. How many observation sites are used to generate the plot in Fig. 8a? Are these sites reliable? The plot appears fairly noisy.

Authors: The figure is generated from 228 automatic weather stations across South Africa. The surface data was regridded using the Cressman objective analysis scheme which is described in detail by Cressman (1959) . A Cressman objective analysis is performed on the station data to arrive at a gridded result that represents the station data. Model grid points are 4.4 km apart, while regridded surface data points are 25 km apart. Hence it appears coarse.

Reviewer: 8. Likewise, how representative and reliable are the three sites used to create Fig. 8c?

Authors: Unfortunately these are those closest to the storm track. We do have a figure indicating that there is consistency between different station.

REFERENCE:

Bryan, G.;Wyngaard, J.; Fritsch, J. Resolution Requirements for the Simulation of Deep Moist Convection. Mon. Weather Rev. 2003, 131. doi: https://doi.org/10.1175/1520-0493(2003)131%3C2394:RRFTSO%3E2.0.CO;2

Cressman, G. P., 1959: an operational objective analysis system. Mon. Wea. Rev., 87, 367–374, https://doi.org/10.1175/1520-0493(1959)087<0367:AOOAS>2.0.CO;2.

Keat, W.J., Stein, T.H.M., Phaduli, E., Landman, S., Becker, E., Bopape, M-J.M., Hanley, K.E., Lean, H.W., and Webster, S.: Convective initiation and storm life cycles in convection-permitting simulations of the Met Office Unified Model over South Africa, Quarterly Journal of the Royal Meteorological Society, 145, 1323-1336. doi: https://doi.org/10.1002/qj.3487, 2019

Markowski, P. M., 2002: Hook Echoes and Rear-Flank Downdrafts: A Review. Mon. Wea. Rev., 130, 852–876, https://doi.org/10.1175/1520-0493(2002)130<0852:HEARFD>2.0.CO;2.

Markowski, P.M., and Richardson, Y.P.: Tornadogenesis: Our current understanding, forecasting considerations, and questions to guide future research, Atmospheric Research, 93, 3-10. doi: https://doi.org/10.1016/j.atmosres.2008.09.015, 2009

Ndarana, T, Mpati, S., Bopape, M-J.M., Engelbrecht, F., and Chikoore, H.: The flow and moisture fluxes associated with ridging South Atlantic Ocean anticyclones during the subtropical southern African summer, International Journal of Climatology, 1– 18. doi: https://doi.org/10.1002/joc.6745, 2020.

Roberts, N. Assessing the spatial and temporal variation in the skill of precipitation forecasts from an NWP model. Meteorol. Appl. 2008, 15, 163–169. doi: https://doi.org/10.1002/met.57

Weisman, M.; Skamarock, W.; Klemp, J. The Resolution Dependence of Explicitly Modeled Convective Systems. Mon. Weather Rev. 1997, 125. doi: https://doi.org/10.1175/1520-0493(1997)125%3C0527:TRDOEM%3E2.0.CO;2